

Dieser Wulst geht von den Septen auf die Wände des schwammigen Gewebes, das, wie gesagt, den Körper der Colonie selbst bildet. In dieser Weise ist das Innere des *Veretillum*-Stammes von den erwähnten Wülsten durchzogen, was eine beständige Bewegung des Wassers, einen Kreislauf, bewirkt. Da aber in den Wänden, weder der Colonie selbst, noch der geschlechtlichen Polypen, wäre es nicht der Mund selbst, keine anderen Öffnungen zu finden sind, so müssen wir den geschlechtslosen Polypen eine Aufnahme und Abgabe des Wassers zuschreiben. Hier ist noch zu erwähnen, daß die Mundplatte jedes Polypen von Nervelementen, deren Empfindlichkeit die Ausstülpung des Oesophagus bewirkt, versehen wird; es kommen hier auch kleine Leuchtzellen vor, die einigermaßen eine Phosphorescenz, obschon wohl viel schwächere, hervorrufen.

Etwas über Geschlechtsproducte. Die großen geschlechtlichen Polypen sind alle männlich; die Eier bilden sich aber im Stamme des *Veretillum* selbst, und kommen in der Form von vier Längssträngen vor, die äußerlich an vier Seiten des inneren Achsencanals angebracht sind.

Da die Eier näher zu den ungeschlechtlichen Polypen stehen, so kann man vielleicht annehmen, daß ursprünglich alle Polypen geschlechtlich waren; mit der Zeit aber reducirten und veränderten sich die früheren Functionen, die weiblichen Geschlechtsproducte rückten in's Innere der Colonie, was endlich eine Entstehung von geschlechtslosen Polypen hervorrief.

Villafranca, 20. Juni 1857.

4. The Reproduction of Myxine.

By J. T. Cunningham in Edinburgh.

eingeg. 23. Juni 1857.

Prof. Max Weber (Zool. Anz. No. 253) assures me that his purpose in his communication on *Myxine* at a certain meeting of the Nederl. Dierkundige Vereeniging was to make known to his fellow zoologists the results of my work, and to express his satisfaction at, and agreement with my observations. I gladly accept the assurance, but at the same time I cannot understand what significance he attaches to the sentence, which he quotes from himself, »Onderzoekingen van W. Müller, die reeds het meerendeel der resultaten, doer C. verkregen, in nuce bevatten.« I translate it thus — »which contained in nuce the greater part of the results furnished by Cunningham.« What is meant by »in nuce?« As I have shown Müller's paper in its account of many points which I dealt with was erroneous, while of many other important matters which I elucidated it gave no account

at all. It still seems to me that I was entitled to object to the statement that Müller's paper contained the greater part of my results in nuce, or in any other manner. I do not agree with Prof. Weber that my merit would have been the same if I had simply rediscovered Müller's results independently. A scientific investigator almost daily, independently discovers things which as he afterwards finds have been made known before, but he takes no credit for, and does not if he can avoid it publish such rediscoveries. As Prof. Weber's says everyone can by comparing the two papers find out what was Müller's contribution, and what mine, to our knowledge of the sexual organs of *Myxine*.

With reference to the ova in the Göteborg Museum I have one or two things to say. The date 1884 quoted in my remarks in the Zool. Anz. as that of the taking of these eggs was a printer's error, or possibly a slip of my own. I meant to write 1854 as it stands in the report of Prof. Weber's communication. But with respect to the date »5. August« given on the label of the specimens in question, Dr. Anton Stuxberg informs me by letter that the late Professor A. W. Malm says in his »Vertebrate Fauna of Bohuslän«, that the ova were presented to the Museum of Göteborg in Aug. 5 1854, and that they had been found the year before: that they were taken from the stomach of a cod, but as to the time of year when they were found nothing is known, and the person who found them has been dead several years. It follows therefore that no conclusion whatever can be drawn from the label of the Göteborg specimens as to the time of year at which the oviposition of *Myxine* takes place. With regard to Prof. Weber's opinion that the time of year is at Alvärströmmen about October or November, I pointed out that the evidence he went upon, as he himself will readily admit, was inconclusive. I wished to insist upon the almost certain criterion of the occurrence of oviposition at a given time, which I had discovered, namely the capture of recently spent females. This year I have taken several females in this condition in April in May and in the first half of June, so that there are now 6 months of the year, Nov. to June inclusive, in which it is in my opinion certain that eggs of *Myxine* are deposited near St. Abb's Head. I ought not to have spoken on a previous occasion of oviposition being limited to a certain time of year. I meant that it was proved to take place during and to extend throughout a period mentioned. I now expect to find that recently spent females can be captured in any month of the year, and that oviposition is therefore not limited to a particular season.

As to the question of hermaphrodites and males the probability of all females being hermaphrodite when young was the least impor-

tant of my results. I understand Prof. Weber to doubt the statements I had made concerning the abundance of hermaphrodites and the occurrence of living active spermatozoa in them. The report of Prof. Weber's communication which I received implied such a doubt, which, if it existed, could be dispelled by the examination of any dozen *Myxine* taken at random.

On the question of male efferent ducts in Teleostei I frankly confess to want of information, and on this point I have already made a complete acknowledgment to Prof. Weber privately.

I think I need not apologize for continuing this controversy. The personal question which is of course of little interest to anyone but myself, is not the major part of it, and I think no apology will be expected by the editor of the Zool. Anz. for discussing the question of the period of oviposition of *Myxine* when it is remembered that abundant as these animals are on the coasts of the North Sea deposited eggs have been obtained only twice, and we are still completely ignorant of the conditions in which the eggs are placed when undergoing development, and of means by which they may be obtained again.

III. Mittheilungen aus Museen, Instituten etc.

1. Filz-Eiweißplatten zur Befestigung zootomischer Praeparate.

Von Dr. H. Dewitz in Berlin.

eingeg. 26. Juni 1857.

So elegant die Glasplatten sich ausnehmen und so brauchbar sie für viele Sachen (z. B. ganze Thiere) sind, so leiden doch feine Praeparate (die inneren Theile von Insecten, Mollusken etc.) gewaltig während des Trockenlegens und Aufklebens. Flottirende Theile, z. B. die Malpighischen Gefäße legen sich der Platte an und schwimmen nie mehr.

Die Herstellung und Anwendung durchbohrter Glasplatten ist für vielfach verzweigte Praeparate zu umständlich. Holz- und besonders Wachsplatten werden von Alcohol angegriffen.

Nach jahrelangen Versuchen (mit Leber, magerem Käse, getränkten Holz- und Torfplatten) ist es mir gelungen, Platten zu verfertigen, welche nach meinem Dafürhalten allen Ansprüchen genügen. Dieselben werden aus weißen Filzstücken, welche man mit Eiweiß trinkt, hergestellt.

Hühnereiweiß wird vom Gelben abgossen und einige Tage an einem warmen Orte, z. B. am Herde in sehr dünner Schicht in flachen Tellern gehalten, bis es recht dickflüssig geworden ist. Will man es

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